Author's response to reviews

Title: Injury morbidity in an urban and a rural area in Tanzania: an epidemiological survey

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Author's response to reviews: see over
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The Editor  
BMC Public Health

Re: Paper entitled “Injury morbidity in an urban and a rural area in Tanzania: an epidemiological survey (MS: 1020804133474990)

Thank you for your email of 8th November.

We thank the reviewers for their positive and helpful reviews. We have addressed their comments and have revised the above named paper as suggested. A point-by-point response to each of their comments is provided below.

Yours sincerely

Candida Moshiro
Reviewer One: Olive Kobusingye

1. Severity of injury is classified as minor if resulting in less than 30 days of lost activity and major if resulting in more days of lost activity. The basis of this severity categorization is unclear. One can imagine that a lot of truly minor injuries that only slowed a child down for two days are lumped up with those which might require a splenectomy, from which operation a child will fully recover and be back in school before the month is out. Most previously healthy young people will recover from severe injuries in two or three weeks, if they receive prompt and appropriate care – except for severe extremity injuries or burns. Since this choice of cut-off affects the rest of the analysis, it is worth either considering different, previously validated measures of severity, or at the very least explaining the rationale for this categorization.

_We have included an explanation on the rationale for the categorization of severity of injury in the methods section on page 6. Our choice of categories is related to the different recall of injuries observed in the separate severity groups._

2. Introduction and literature review, page 3:
The authors say that “little is known about the causes and groups at high risk of injury in low income countries, especially in Africa.” I consider this to be untrue, given what is available in the literature on the subject, especially over the last 10 years. What may not be well known is what interventions work well to reduce risk, but the causes and risk groups are well known, even from the references given by the authors. In fact, two of the papers cited later are also population based:
The authors may also want to look at work by Alex Butchart and others on the epidemiology of intentional injuries, and Samuel Forjouh on burns and other injuries.

_We agree with the reviewer and have removed the sentence. We had already included one of Samuel Forjouhs’ paper on risk factors for childhood burns._
3. Results:
First paragraph concerning the description of the study population. It would be good to tell readers if the observed proportions of persons below and above 44 years are similar to national averages.

_We agree with the reviewers’ suggestion, and we have added one brief sentence towards the end of the first paragraph in the results section on page 7._

Types of injuries recorded: Drowning and poisoning are conspicuously absent. Were they not seen at all, or were they not included in the range of injuries? Dar es Salaam being a port city has extensive water surface, and if indeed there were no drowning seen, it should be commented on as an important negative finding.

_Near drowning and poisoning were included in the list of injuries. There may have been incidents of drowning that led to death but it was not part of this study. We have included a statement underlining the point (para 2 page 12)._  

4. Discussion:
Page 12, second paragraph: “Primary education was found to be associated with an increased risk of injuries.” This statement is subject to varying interpretations – as it stands, it makes the attainment of primary education a risk factor. But it could also mean that the lack of post-primary education is a risk factor for injuries. The authors should write it so as to avoid ambiguity.

_We have modified the statement accordingly (page 13 paragraph 2)._  

5. Third paragraph, second last sentence: “... the severity of injury could not be assessed anatomically by retrospective self-reporting.” This may be true, but it makes it sound like anatomical assessment is the gold standard. In fact, there is no such agreement on the measurement of injury severity, and some of the tools that are widely used have both anatomical and physiological parameters. A common example is the Revised Trauma Score. It is probably better to say a clinical injury severity assessment was not possible.

_We agree with the reviewer and have modified the sentence accordingly (para 2 page 14)._
Reviewer Two: Marilyn Leff

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

A. Methods section
   1. Why did the authors choose to under-represent the first two initial surveillance branches in the urban areas? How might that influence the estimates of injury in the urban area?

   When the survey started in the first two branches, fewer injury events than expected were recorded. We chose to increase the sample size in the remaining six branches in order to get more injury events. The households were randomly selected from each branch, so we felt that we should not go back to the area that had already been covered. There is no reason to believe that the estimates of injury would be affected. Simple tabulation of prevalence of injury in the branches did not indicate heterogeneity.

   Are the first two surveillance branches different in any way from the last six?

   The first two branches are areas of fairly low socioeconomic status where a substantial proportion of residents lead a peri-urban life. However, they also contain people of middle class income.

   How might this selection process influence the ability to generalize to the urban area?

   There is no reason to believe that this process has markedly affected our overall results. We feel it is best simply to describe how the actual selection of branches was made in the methods section and let the reader judge to what extent the results might be affected.

   2. I have the same questions on the purposive selection of the rural villages. It appears that these villages were picked to have a cross-section of different levels of socio-economic and injury status.

   How different are these 6 villages from the other 45? What are the implications for generalizing to all villages in the Hai rural surveillance area?

   In addition to socio-economic levels being similar, the distribution of age, sex and education is comparable between the 6 villages and the other 45 villages. The villages also cover areas of high and lowlands, and areas that are close to highways. We therefore believe that the sample is selected from areas that are likely to be representative of the rural surveillance area.
3. Was forward, backward, or some other means used to develop the odds ratios when doing the multiple logistic regression? From your description and table 3, I don’t understand why you adjusted for the variables you did. On what basis were those decisions made? Were there other variables you investigated but didn’t report? Had you made these decisions before you did the analysis? The authors did not report the independent variables investigated in the methods section.

The independent variables, which were selected a priori, were all entered into the regression model. These were considered basic variables that would be expected to affect the rates. We used neither the forward nor backward method of regression. Variables that we investigated but did not report were marital status, religion and occupation. We have now included a statement about this in the methods section (para 3 page 5). We decided not to include poverty as a potential confounder but rather as an exposure variable adjusting for the other factors, because the information on poverty was essentially at a different level from the other variables.

4. I had to read the following sentence in the second paragraph of the Statistical analysis section several times before I understood it: We found that the pattern of the pattern of associations was similar to that based on all injuries except for area of residence where the size of the effect was stronger when a short recall period was use. I think this belongs in the discussion with some information about what we might expect to find if using only those injuries within a 3 month period and why a 12 month recall was chosen.

We agree with the reviewers’ suggestion. We have moved the paragraph to the discussion section and expanded the explanation (page 13 paragraph 3).

5. Since you could adjust for the cluster design, why not just do it? You explain that the standard errors did not change substantially, but why go ahead without adjustment when you can?

We have included adjustments for cluster design in the analysis. Tables 3 and 4 have been modified accordingly.
Results
1. When determining the injury incidence does that estimate take into account multiple injury events during the past year for those individuals who had more than one event? I think an important finding would be the prevalence of major injuries by region. Does that compare to the difference seen in all injuries? You note in the discussion that the severe injury rates were the same for urban and rural but don’t report the findings in the results.

*We did not consider multiple injury events in all analysis, only the most recent ones were included. This has been clarified in the section on statistical analysis (page 6). We have included the rates for minor and severe injuries in the manuscript (last para page 7).*

2. The authors report that children aged 5 to 14 had slightly higher odds of sustaining a minor injury……etc. You go on to say that you found this difference by major and minor injury by comparing trends with age for major and minor injury. You did not make reference to this procedure in either the methods or the table. Did you look for trends in other categories as well?

*We only looked for trends in the categories that appeared to have one, as the other categories would not show any differences. We have included a sentence about this procedure in the methods section (para 2 page 6).*

Discussion
1. The authors point out that there is a higher risk of transport injury in the urban compared to the rural areas. However, I think it should be noted that while the numbers may be small, transport injuries are more likely to result in major injuries to rural people than the most prevalent type of injury, i.e., cuts/stabs. So that transport injuries, while less prevalent in rural areas, have more dire consequence than some other types of injury.

*We agree with the reviewer. A brief sentence has been included (para 2 page 11).*

2. I think an unanswered question in this study is the high rate of severe injury from falls in both urban and rural areas. This is not mentioned in the discussion; it is only mentioned when talking about age differences in types of injuries. It would seem to me that understanding how those falls take place and distinguishing severe from non-severe falls would be important area of research.

*We have added a comment to this effect (para 2 page 12)*
3. The discussion needs some mention of the use of a 12 month recall period and its affect on the findings (See comment on methods). I would put the information that you found about the rural-urban differences found when using a 3 month recall that was in the methods section in the discussion. By using the 12 month recall period, were the results an under-estimate for rural areas more so than urban areas? So, is the difference you report conservative?

*We appreciate the reviewer’s comment and have modified the text accordingly in the discussion (para 3 page 13)*

4. Do you have any information to know if these patterns of non-fatal injuries are similar to those of fatal injuries in these regions?

*The patterns of nonfatal injuries are quite different to those of fatal injuries. We have included a comment in the manuscript (para 3 page 12).*

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. I would like to know more about the active reporting system and probable cause of death as determined by a validated verbal autopsy. Since the reference for that is an administrative report, it is more difficult to get information on this process. Perhaps this report is on a web-site and could be referenced for readers and thus easier to obtain.

   *The reports can now be found on the following website: http://www.ncl.ac.uk/ammp/site_files/public_html/finrep/. The URL has been included in the reference list.*

2. In the text, you report statistically significantly different findings between urban and rural areas and between men and women within areas as reported in Table 1. Yet, those findings are not included in the table. The table should be able to stand alone and thus would be helpful to show those statistical findings on the table as well.

   *We could not find a way of including the relevant p-values in the table. In practice the comparisons are only made for the largest injury groups of primary interest. It would complicate the table if the results of just these tests should be given among the other results.*

3. The figures did not have titles. Either that is a problem with my printer or they need to be added

   *Titles were in body of manuscript*
3. Table 2 is a little confusing. Under “Both sexes” “Males” “Females” I would put % of injuries that were major instead of (%). Or you could put the number of major injuries next to the “no of all injuries.” The authors report on statistical differences in proportions between males and females in referring to Table 1; did you find any differences in the data in Table 2?

The table has been modified accordingly. We preferred not to carry out any significance tests in this descriptive table.

4. I think there is a typo when referring to Table 7 at the end of the results section. I think it should be Table 6.

Corrected

Discussion
1. How would these results be used in Tanzania? Are they being used to develop prevention programs in these areas? On what basis do the authors feel that the results represent other urban and rural areas of the country?

The information generated could be used by district health councils to identify needs for prevention programmes in the areas. At the national level, they could be used for health policy and planning. At the moment they are not being used for planning purposes. Although we might expect similar findings in other areas, the results may not be completely representative of other urban and rural areas of Tanzania. We have now included a statement about this issue in the last part of the discussion.

Discretionary Revisions (which the author can choose to ignore)

While the stacked charts are appropriate in the figures, I find them difficult to read. I would prefer a table to look at the percentages rather than my “guessing” at them with the charts. That is just a personal preference.

We understand that some readers might want more accurate information to be included in a table, but in this case the purpose of the figure was to show the special relation between injury category and age quite clearly. We feel that this particular kind of information is better displayed in a figure.
I don’t understand why the authors included the following sentence when talking about cuts and stabs in the discussion. Lack of recreational facilities for children, especially in high density residential areas could be a contributing factor. Based on Figures 1 and 2, it looks like cuts are less prevalent among 5 to 14 year olds in the urban area. It seems that this sentence was just “dropped” in here. Please explain your thinking a little more thoroughly.

*Injuries due to cuts are one of the major causes of nonfatal injuries in the urban area among 5-14 year olds. It accounts for 29% of all the injuries and is the second cause of nonfatal injury in that group. We have reworded the sentences in the results (para 2 page 8) and discussion sections (para 3 page 11).*